THE COLORADO FOUNDATION FOR RESEARCH IN TUBERCULOSIS

GERALD B. WEBB MEMORIAL BUILDING, AT THE UNIVERSITY OF COLORADO MEDICAL CENTER

4200 East Ninth Avenue
Denver 20, Colorado

February 19, 1957

AIR MAIL

Dr. Joshua Lederberg
Department of Genetics
University of Wisconsin
Madison 6, Wisconsin

Dear Joshua and Esther:

Enclosed you will find summaries of two experiments on UV of HFT lambda, which I long ago promised. I have not written a text summary to go with them because I believe we have already discussed the significant results. When I am able to make new lysates (having troubles) I will repeat them in entirety and also make more extensive analysis, especially of transductions to L_p^+ recipients. I have just returned from three days at Caltech, where I discussed these experiments (as well as my growth experiments) with the people attending the meeting. It seems that Arber has found similar UV results recently. However, I was able to present the dates on these experiments and to get the impression across that the people in Geneva had not come on something new.

The meeting was quite good, although two nights and a day in the desert in the middle of ~~them~~ it made me so tired that I did not do well in discussing my data. Kaiser and Weigle were very pleasant and I am planning to return for a couple of weeks to do some experiments with Weigle, probably on the growth of transducing particles. He is doing quite well and has confirmed most of our observations. He has now isolated HFT 1^-, 2^- with the lambda mutant he is using. This mutant really grows better than ~~the~~ mine we are using. He checked ~~this~~ and found the low titers we get in HFT, so there are really strain differences. I talked with Kaiser, who is both sharp and easy to talk with, and he said that lambda 26 does not form stable lysogenics and that you (or we) should check the origin of the strains so labeled. There is a lysogenizing recombinant between lambda 26 and a better lysogenizing mutant which has the plaque character of lambda 26 and this may be what we have. He was willing to send any stocks requested. If you have not met him, I would suggest it, since he has worked out a great deal of lambda genetics, and has a great deal of information. From the standpoint of coli genetics he has a Jacobian view, but this is chiefly owing to the fact that he has not heard of other data. I hope to see more of him this summer, since his wife comes from Ft. Collins and he will be vacationing there.

I have not been productive of late, since it has been a period of nothing going right. I hope that my trip has changed it all and I again will be able to get results of meaning. I will send along some data (old) on crossing heterogenotes in a few days and try once again to put my data for our papers in final form.

The culture of W1164 arrived broken but we were able (easily) to isolate a Gal-M$^-$ clone from it. The old data suggested that W2281 gave a large number of

$Lp^{R/S}$ transductions with LFT lysates. I wanted to see if this was a property of the Lp^{S} of W2281 or of some other marker in the W1210 line. I planned to isolate Lp^{S} from W1210 and compare them with W2281. However, I have not obtained $Lp^{R/S}$ heterogenotes with fresh lysates on W2281 so that it may be that the old data are explainable on the change of lysates with time. Weigle told me that Arber has looked at HFT lysates under the electron microscope and finds agreement to 30% between plaque titer and particle counts, so that there cannot be many non-plaque forming particles in the lysate (or lysates?) that he looked at. This may be good for the S/S explanation, which I believe Weigle may be considering now. I did not have a chance to talk with him about this.

What is it going to be--Stanford, Berkeley or Madison? Everyone wants to know, I suppose even yourselves.

As ever,

Jamy

M. L. Morse, Ph.D.

MLM/mb
Encls.